he was very eager to get the meat out of it, and that when later in the day he succeeded, he showed no manner of misgiving as to his legal right to its possession.

Other of my correspondents misunderstand the purpose of the experiment. They see in it a desire to belittle their canine pets. This was very far from my thoughts. We have innumerable anecdotes telling us what dogs can do. I wish, partly I admit with a view to enabling us to sort these stories, to obtain, as data, definite observations showing what dogs will not do. Into most dog stories there creeps the little touch of human nature which makes them and ourselves akin.

Mine is the point of view of an anatomist. A dog has a brain very different from that of man. Brain and mind are the two sides of the same coin; or rather, brain is the coin, mind its value. The dog's brain cannot make a man's thoughts. How near can we come to picturing to ourselves the nature of a dog's thoughts? Without committing ourselves to Flechsig's theory of the division of the cortex of the brain into "projection areas" and "association areas," we may on anatomical grounds assert that the cortex of a dog's brain contains fewer association elements than does that of a man. It is an apparatus for transforming sensory impressions into actions, in a more limited and exclusive degree. Probably we can best picture to ourselves the work that it does by supposing that the wordless thoughts of animals are direct combinations of sensory impressions; whereas man has invented symbols for his sensory impressions. He works the symbols into thought. Nor do his symbols stand for material objects alone. They also stand for inferences from observations. But this is a subject which perhaps I ought not to touch without having at my disposal more space than I can ask you to give me in your Journal.

We must admit with Sir William Ramsay that dogs make use, in their mental operations, of sensory impressions and not of inferences, although I dissent from his qualification of their impressions of smell as "vague." It is my object to ascertain, by means, if possible, of observations which can be made under properly controlled conditions upon numerous dogs of various breeds, the limits of their power of substituting inferences for sensory impressions as materials of thought.

Perhaps I may be allowed to use a new nomenclature in defining the position in which, as it appears to me, we stand with regard to the axioms of animal psychology at the present time. An animal remembers. When it performs an action a picture of the action is stored in memory. If the result of the action be satisfactory, a picture of this result is stored in memory. When in future the animal desires to obtain the result it repeats the action. This we may call the product of "reasoning in the first degree." Action depends upon inference. We may accept it as an axiom that an animal can draw an inference of this kind. It is not yet established, by experimental methods, that an animal can combine two inferences, or, as I venture to term it, "reason in the second degree." My box-experiment was intended to throw light upon this question. I shall be very grateful for any further suggestions of possible experiments of the same kind.

ALEX. HILL.

Downing Lodge, May 2.

Spherical Aberration of the Eye.

WITH reference to the experiment described by Mr. E. Edser (p. 559) as appearing to have "escaped observation," perhaps I may be allowed to state that this phenomenon was (to the best of my recollection) described by me before the School Natural History Society when I was a boy at Rugby, about 1873–1874. I could not explain it, and no one at the meeting had any suggestion to make.

I think I connected it in my mind with irradiation phenomena, though I was baffled by the fact that the *whole* line is bent.

If the black horizontal lines drawn between different advertisements on the outside of NATURE be held five or six inches from the eye, and the rounded end of a pen be brought down close to the eye, the whole line will be seen to curve upward to meet the pen, becoming also blacker and more distinct.

W. L.

THE phenomenon mentioned by W. L. must have frequently been noticed; while resembling that described by me as a proof of the spherical aberration of the eye, it is yet due to an essentially different cause. The black line, when placed at a distance of five or six inches from the eye, is within the shortest distance of distinct vision from the latter. A point source of light, situated on the axis of the eye, at a position closer to the eye than the "near point," produces a relatively large spot of light on the retina. If the pupil be now progressively covered from above, the rays passing through the middle and upper part of the pupil will be cut off, so that those passing through the lower part of the pupil alone remain; these cut the retina in a comparatively restricted area below the point of intersection by the axis of the eye, so that the image apparently rises, at the same time becoming more sharply defined. Under the conditions mentioned, the same phenomenon would be observed if the eye were entirely free from spherical aberration. For this reason I stated that the black band should be placed "just beyond the shortest distance of distinct vision from the eye; . . . care must be taken to keep the eye carefully focused on the edge of the black band, or an exaggerated displacement, due to relaxation of the accommodation of the eye, may result." It was merely as a proof of the spherical aberration of the eye that I described this experiment as having apparently escaped observation.

April 12. EDWIN EDSER.

IN connection with the experiment on the spherical aberration of the eye, described in your issue of April 16, I may relate a striking observation I made some years ago. Regard with one eye any light or bright object on the wall, turn the head away until the object is just covered by the line of the nose; then move the eye to its natural position, and the object will reappear, supposing the nose is not too prominent. Moving the eye several times to and fro, the phenomenon will be easily observed.

Leipzig, April 29. W. BETZ.

THE SOLAR AND METEOROLOGICAL CYCLE OF THIRTY-FIVE YEARS.

THE fact that the rainfall of many regions of the earth's surface has, for the last decade or more, been gradually diminishing has led many inquiries to be made concerning the possible periodicity of this meteorological element, and during the last few months more general attention has been drawn to this interesting question. The great importance of this inquiry, not only to agriculturists but to others, renders it desirable that all facts which may tend to elucidate the subject should be thoroughly discussed.

The object of the present article is to bring together, without entering into too great detail, a few statistics relating to the rainfall of different stations in various parts of the earth to see whether there be grounds for assuming a continuation of the present small supply, or whether a greater abundance may be looked for with special reference to the condition of the British Isles.

A few introductory remarks may here not be out of place. Eduard Brückner first discovered that wet periods, great droughts, &c., occurred at intervals of about thirty-five years, and he published his important conclusions in a volume which was, and still is, a valuable contribution to meteorological science. To take one element only, namely, rainfall, Brückner showed that during the last century the mean epochs of the wet years were 1815, 1846–50, and 1876–80, while those for the dry years were 1831–35 and 1861–65.

Since the publication of this volume, many workers have studied rainfall and other records extending over long periods of time. Thus, to take one instance among many that might be cited, Herr Hofrath Julius Hann, the distinguished late director of the Vienna Meteorological Institute, made a minute investigation of the

rainfalls of Mailand, Padua, and Klagenfurt, and found a well-marked recurrence of the wet and dry periods every thirty-five years, the mean epochs of the former being 1808, 1843, and 1878, and of the latter 1823, 1859, and 1893.

In determining the variation of rainfall over such long periods as that of thirty-five years, it is necessary, if possible, to smooth the curve representing the variation from year to year, for this curve, as a rule, displays large fluctuations from the normal in the course of a very few years, and it is not easy for the eye to grasp the longer periods of variation; these long periods may to some extent be rendered more apparent by coupling up together the mean values of the rainfall for several years, and forming another mean, but somewhat fictitious value, for each successive year. Thus, for instance, the mean for one year, say 1870, might be computed from the means of the five years 1868 to 1872, or the means for 1871 from the mean of the years 1869 to 1873; instead of a five-year mean, a ten-year or a fifteen-year might be chosen.

In the figure here given, five-year means have been adopted, and the curves resulting from these have been further smoothed by drawing freehand another curve to eliminate as far as possible the smaller fluctuations of short period that still exist, even after still minor changes have been eliminated. The stations, the rainfall curves of which are here given, have not been specially selected, but simply taken as the data for them were easily available, and they afforded long records for the study of such variations as are here discussed. The short curve for the British Isles is attached so that not only can a comparison be made of this record of the Meteorological Office with that obtained by the late Mr. Symons, but that the actual variation over the islands taken together can be compared with two widely separated stations in them, as Greenwich and Rothesay. The European continent is here represented by Brussels, the epochs of the maxima and minima of the rainfall curve of which can be compared with the values given by Hann and referred to in a previous paragraph. Two stations in India, Bombay and Madras, one station in South Africa, Cape Town Observatory, and lastly three stations in the United States of America representing the rainfall of the Upper Ohio Valley, complete the rainfall information here given.

A general collective glance at these curves shows that there is an undoubted long period variation in all the stations here brought together. Further, that the periods of greatest rainfall occur *generally* in the years 1815, 1845, and 1878–83, while those at which the rainfall is decidedly deficient are about the years 1825–30, 1860, and 1893–5.

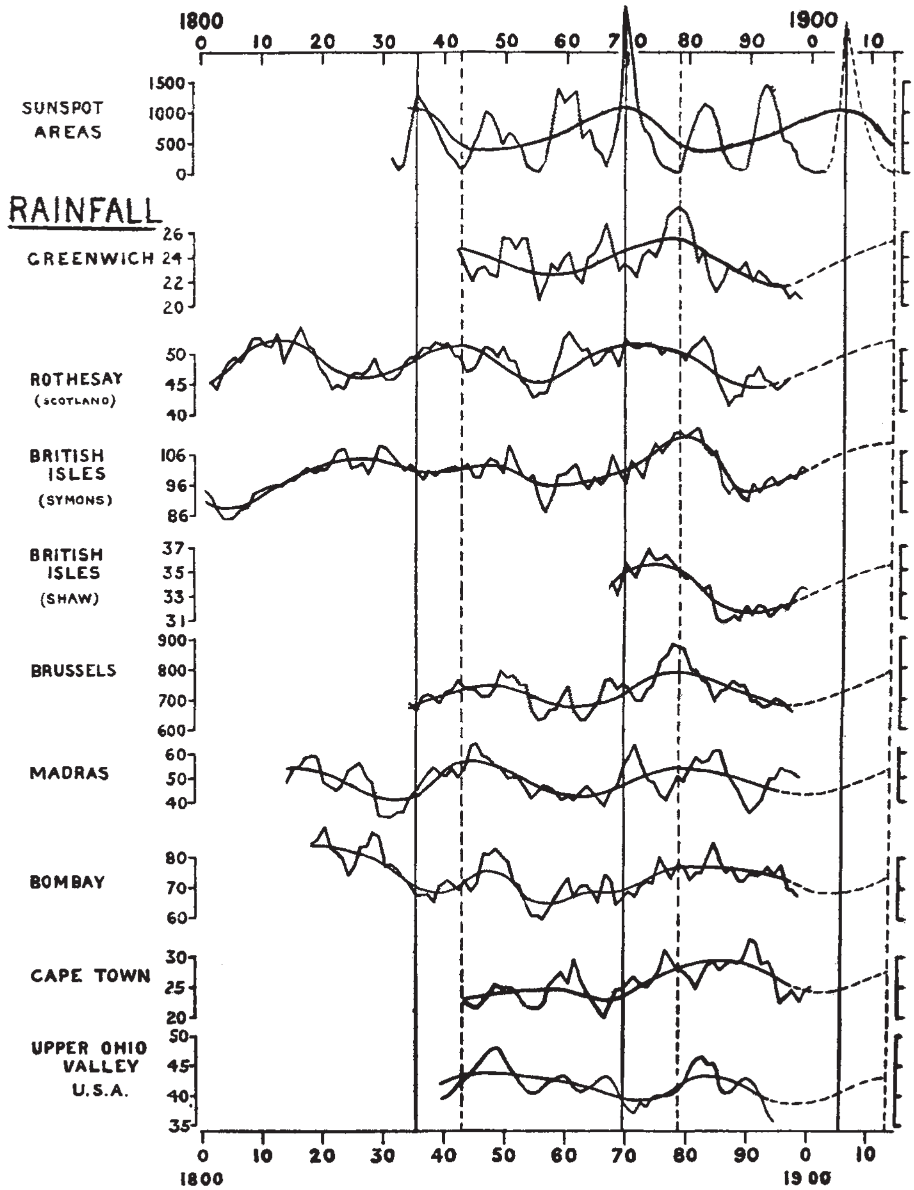

FIG. 1.—Curves showing the relation between the 35-year sunspot period and that of the Brückner rainfall cycle. Each of the rainfall curves is determined from the means of five-years, and these curves are smoothed by freehand drawing in order to show the long period variation of rainfall. The smoothed curve through the eleven-year sunspot curves indicates the epochs of the long period sunspot variation.

With the existence of these very definite fluctuations it is important to notice that the last minimum or dry period which is most apparent in the case of the curves representing the British rainfall seems now to be just

past, or on the point of coming to a conclusion, and in all cases the general tendency of the long period curve is now to rise again. This indication of the increase of the rainfall is represented in the figure by the dotted continuation of the secular variation curves for each station, and should the apparent law hold good, there seems sufficient evidence to mark that this rise will continue to take place until about the year 1913, which year will suggest the middle of the next wet epoch.

It may be mentioned, however, that owing to the great oscillatory nature of the rainfall from year to year, this rise only represents the mean rise when several years are coupled together; there may be comparatively dry years even when the secular variation curve is at a maximum, but on the average they will probably be wet.

What causes this long period of weather variation is not yet definitely known, but it is of the highest importance to meteorological science that the matter should be cleared up as soon as possible, for not only is our rainfall involved, but all other meteorological elements show similar fluctuations.

Brückner attempted to account for this long period weather cycle by attributing its origin to a change in the activity of the sun, and he investigated the sunspot data then available for evidence of a periodicity of about thirty-five years. He was not, however, successful in his research, but he concluded that, although this variation must really exist in the sun, yet it might not necessarily be indicated by sunspots. More recently a minute examination of the sunspot observations made since the year 1832, when a systematic method of observation had been initiated, has led to the discovery of such a period, a detailed account of which appeared in a previous number of this Journal (NATURE, vol. lxiv. p. 196). It was there shown that each sunspot period (reckoning from minimum to minimum) differed in many respects from the one immediately preceding or following it. Some periods, for instance, were not only more "spotted" than others, that is, the summation of the whole spotted area from one minimum to the next varied regularly, but these particular periods were closely associated with comparatively rapid rises from minimum to maximum in those periods. These changes further seemed to be undergoing a regular variation, the cycle of which was determined to be about thirty-five years.

The connection between Brückner's cycle and this long period solar change of thirty-five years was there briefly stated, and it was shown that at those two epochs of sunspot minima, namely, 1843 and 1878, which follow the cycles of greatest spotted area, the Brückner rainfall cycle was at a maximum.

The close correspondence of the epochs of these two cycles suggested at once a probable cause and effect, a cause which Brückner himself had suggested and looked for, but unfortunately did not find.

In the accompanying figure the uppermost curve represents the sunspot curve from the year 1832, and the minima just referred to are indicated by the vertical dotted lines, which are continued through all the curves. The periods of greatest spotted area just precede these epochs, and the times of maxima are shown by the vertical continuous lines drawn in a similar manner. To show the probable times of the recurrence of these epochs during a portion of the next great period of thirty-five years two vertical lines have been inserted at the years 1905, which is the probable epoch of the next great maximum, and 1913, the following minimum, so that their relation to the probable variation of rainfall, as indicated by the dotted portions of the curves, can be seen at a glance.

In conclusion, attention may be drawn to the fact that during the last few years a far more close connection between solar and meteorological phenomena has been made out than was the case some years ago, and since this long period rainfall cycle synchronises so well with the solar changes, the latter may render valuable assistance in determining the epochs of these dry and wet cycles. WILLIAM J. S. LOCKYER.

ETIOLATION.[1]

THIS monograph is published by the aid of the Daly Lydig fund bequeathed by Charles P. Daly, and embodies the results of the author's investigations extending from 1895 to 1902, and one of the first questions it arouses is, to what extent is this sustained experimental work stimulated by the certainty of adequate publication owing to the generosity of patriotic endowment, and to what extent does such work react on the pockets of friendly millionaires and induce the endowments for further work? In any case, Americans are fortunate in their circumstances in these respects.

The book, which comprises more than 309 pages of text and 176 figures, all admirably done, is divided into three principal sections. There is, first, a summary of the history of the subject, beginning with Ray (1686) and Hales (1727), and occupying 34 pages of more or less critical notes. It is, of course, impossible for us to verify the enormous number of the references to this part of the subject, but if the author has made many such blunders as those on pp. 27 and 29, where on two separate occasions he cites volumes and pages as from *Proc. Roy. Soc.* when he should have written *Philosophical Transactions*, the value of his bibliography must suffer. If a leading American plant physiologist does not know the difference between the two publications referred to, it is time he did; if he *does*, the inference that he has not consulted the original memoirs is as inevitable as it is dispiriting.

The second chief division of the work occupies the bulk of the book, pp. 35–200, and reflects credit on the author and his pupils for their industry and clearness of description, as well as for the interesting choice of plants selected for experiment. These include not only ordinary flowering plants, but also more out of the way forms of monocotyledons and dicotyledons, as well as ferns, Equisetum, &c. The one note of disappointment in this portion of the book will be struck by the want of plan. Numbers of most interesting observations on the behaviour of particular species in the dark, and illustrations of their facies, their anatomy compared with that of normal plants, their curves of growth and so forth will make the book useful to all investigators; but the plants are arranged in alphabetical order, and when the reader turns to a particular species he has no guide as to how it will be treated. Thus, taking at random Salvia, Sansevieria, Sarracenia, Saururus, and Sparaxis, which follow in the order given on pp. 171–180. The first merely heads a small paragraph stating that the corolla is atrophied in darkness. Under Sansevieria the etiolation of the shoot is described only in so far as external changes are concerned. In Sarracenia the effects of etiolation on the histology of the epidermis lining the "pitchers" are well illustrated. In Saururus figures of the anatomy of etiolated and normal stems, and measurements of height and thick-

[1] "The Influence of Light and Darkness upon Growth and Development." By D. T. Macdougal, Ph.D., Mem. New York Bot. Garden, Vol. ii. Pp. xiii + 319. (1903.)